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namely, (1) by pairing the yellow mice with each other, and (2) by crossing them back with pure grays, blacks or browns. In either case, pure or homozygous F_2 yellow mice (or in Professor Morgan's view those that contain only 'latent' as opposed to 'free' gray) should give only yellow offspring, owing to the uniform dominance of yellow, while mixed or heterozygous yellows (yellow mixed with 'free' gray) should produce grays or other colors as well as yellows. Cuénot says: 'J'ai essayé par l'une et l'autre méthodes un nombre considérable (81) de Souris jaunes * * *.' 'Or, a mon grand étonnement, je n'en ai pas trouvé une seule (*i. e.*, homozygote).' Professor Morgan considers this statement, which embodies the principal result of Cuénot's experiments, as 'somewhat ambiguous,' apparently for the reason that Cuénot does not in this passage actually use the words that all the yellow mice produce offspring of other colors as well as yellows; but it must be obvious that only such a result could justify his statement, and if this be not Cuénot's meaning I am unable to discover any meaning in his paper. In point of fact, however, he states specifically on a preceding page (cxxvii) that the cross between a yellow mouse and a pure-bred one of a different color always gives offspring of this color (gray, black or brown) in addition to yellows, the numbers being stated, in the case of the yellow-gray cross, to be equal.

Now, according to Professor Morgan's assumption there should on his own showing be two classes of yellow mice in F_2 , of which "the first group $CY(CG)$ (*i. e.*, those containing 'latent' gray) will breed true, the other group $CY(CG)(CY)CG$ (containing 'free' gray) will split up in each successive generation according to the Mendelian formula." Such a behavior of the F_2 yellow mice was precisely Cuénot's expectation, but 'to his great astonishment' it was contradicted by the results. Professor Morgan, nevertheless, insists that the case of the yellow mice is precisely similar to that of extracted gray dominants (both being 'contaminated' by the recessive character in the latent condition) though, as Cuénot was the first to show, the

latter breed true save for the rare appearance of a different color, such as black, probably derived from the latent color of the original albino used. If the two cases do not differ, why was so experienced an observer as Cuénot astonished at his results, and why did he go so far out of his way to construct the special hypothesis of selective fertilization to explain the behavior of the yellow mice as distinguished from those of other colors?

The difficulty with Professor Morgan's explanation is that it proves too much, for it explains the special peculiarities of the yellow mice out of existence (!). My criticism is not directed against Professor Morgan's general assumption, but I think that it entirely fails, as far as he develops it, to account for the peculiarities of the yellow mice, and that it leaves Cuénot's hypothesis, which it is supposed to obviate, exactly where it stood before.

E. B. WILSON.

THE LOGICAL BASIS OF THE SANITARY POLICY OF MOSQUITO REDUCTION.

THE excellent address of Sir Ronald Ross under the above title published in the December 1 number of SCIENCE, states the general rules regarding mosquito distribution with great accuracy; but it applies only to certain species, including the *Anopheles* so far as known to me and to *Stegomyia fasciata*. It does not apply in the least to such forms as *Culex cantator* and *C. sollicitans*. None of the suggestions as to erratic flights that practically restrict the distance traveled influence these species, which are truly migratory and are guided by some motive other than finding food or a place to breed. In fact, as I have shown, these migrants never propagate their kind and where they are to be dealt with, all of the carefully reasoned mathematical deductions fall. The matter is of great practical importance in New Jersey where communities within whose boundaries not a mosquito breeds, nevertheless, sometimes find life a burden because of the insects. Local work in such cases is worse than useless. When we find the dominant mosquitoes in the Orange Mountains to be species whose nearest breeding place is on Staten Island, time and money

spent in the Oranges would be obviously wasted.

As applied to the usual inland species the argument made is fully borne out by my field experience. As to the salt marsh breeders it is utterly inapplicable—witness the fact that the work done on the Newark meadows resulted in a marked decrease in the mosquito troubles at Paterson many miles to the north.

JOHN B. SMITH.

NEW BRUNSWICK, N. J.,

December 15, 1905.

YELLOW FEVER AND THE PANAMA CANAL.

TO THE EDITOR OF SCIENCE: The continuous discussion of Panama Canal affairs suggests to me to call attention to the possibility that the cutting of the canal may lead to trouble from yellow fever in two of our Pacific island colonies. In the summer of 1902, spent in the Hawaiian and Samoan islands as agent of the U. S. Bureau of Fisheries, my attention was forcibly called to the unusual proportions of the mosquito plague in both these island groups. If it were not for the dragonflies which wage effective war against the 'day mosquitoes,' and for the bed canopies of netting which protect the sleeper from 'night mosquitoes,' life would hardly be tolerable in Honolulu. In Tutuila (our principal Samoan island) mosquitoes are the most obvious features of the above-water fauna aside from the brown natives themselves. Now both in Hawaii and Samoa one of the most abundant of the infesting mosquito species is *Stegomyia fasciata*, which is none other than the yellow-fever mosquito, that is, the particular mosquito species which harbors and disseminates, in yellow fever regions, the plasmodium or bacterium which is the immediate cause of the disease.

So far no cases of yellow fever have occurred in Hawaii or Samoa, but this is obviously not because of the absence of the yellow fever host, but, presumably, of the yellow fever specific causal agent, the pathogenic 'germ.' It is to be presumed that ships have not yet carried yellow-fever-germ-infested specimens of *Stegomyia* from the West Indies to Hawaii or Samoa. Going round the Horn

is probably an effective check to the spread of yellow fever from the West Indies to our Pacific Islands by reason both of the time required and the low temperatures met. Besides there is little traffic now between the two regions. But with the cutting of the canal, making possible a direct short-time passage of ships from the Gulf of Mexico to Hawaii, or to Samoa, all of the voyage being within tropical or subtropical latitudes—the Hawaiian islands are in 20° north latitude, the Samoan islands in 14° south latitude—will there not be a real danger of planting the dread agent of yellow fever in our Pacific colonies in which already the necessary insect host exists in enormous numbers? There may be obvious reasons why this migration can not take place, but they are not apparent to me now. It is, at least, a contingency to be had in mind by those charged with the responsibility of public health affairs in Hawaii and Samoa.

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REPORT OF THE TENTH GEOLOGICAL EXPEDITION OF HON. CHARLES H. MORRILL, SEASON OF 1905.

THE season of 1905 marked a renewal of paleontological activity in the University of Nebraska, since it so happened that for the first time in several years funds became available again for the prosecution of such work.

By virtue of the liberal support and patronage of Hon. Charles H. Morrill, of Lincoln, annual geological expeditions, essentially paleontological in character, had been maintained in connection with the state university since 1892. In 1901, though his interest in the work as well as his good will continued, his patronage ceased. This was wholly due to the overcrowded condition of the state museum, coupled with unusual fire risks, which plainly endangered public and private collections. In the meantime the work of making general collections has been pushed by the state survey, but the special work conducted by the annual Morrill geological expeditions was necessarily of a desultory order, the expenses being met by the sale of duplicate specimens.